for almost all λ

Oct. 11, 1990

Dear Michael,

Many thanks for your letter, which made me think over my argument with Clifton. I now retract the sentence in the second paragraph of my letter to him of 22 Sept., "But since ϕ is arbitrary we have proved that the result function A (x) is indeed a constant function in the real variable x." That conclusion is wrong, for just the reasons that you have laid out in your letter. Needless to say, I also retract the testiness of my paragraph. I am apologetic and chastened.

Let me restate the crucial points in GHSZ, leaving out details which we never disagreed on, and you will see exactly what I maintain. First, (11a) is meant to hold for almost all λ , not for all λ as you attribute to us. One of the footnotes (I believe 12, but I don't have the ms. at home to check, and furthermore you may not have the most recent version), specifically says this. Given (11a), thus interpreted, it does follow that for each ϕ, $A_\lambda(2\phi)=A_\lambda(0)$ -- which is the only part of Eq. (16) that we need for deriving the inconsistency of premisses (i), (ii), (iii), and (iv). But we added another clause in Eq. (16), which says that for a fixed (except for a set of measure 0 in λ), $A_\lambda(2\phi)$ is a constant. And that is wrong, just as you say. In other words, what you write in your letter as (16*) is correct, and what you write as (16) is incorrect -- I agree with you completely. A few samll changes are needed in the lines following Eq. (16) to eliminate any indication that $A_\lambda(2\phi)$ is a constant in ϕ. What I continue to maintain is that our proof is correct for what we were aiming at: namely the inconsistency of the premisses, for we obtain that for almost all λ a certain product of outcome functions is both +1 and -1 -- and we use rigorous set theory in deriving this result. For only a finite number of choices of settings is required for the purpose, hence the strictures about the measure of a nondenumerable intersection of sets of measure one are inapplicable. In the original version of your paper, it seemed to me then and still seems to me, you invoked this stricture to criticize the main proof, which does not depend upon the last clause of our Eq. (16), asserting the constancy of $A_\lambda(2\phi)$ in ϕ.

The galley proofs of GHSZ came from Am. J. Phys. just at the time of your letter, and I therefore had an opportunity to strike out the offending clause in Eq. (16) -- fortunately, a minor change in the galleys, even though it is important conceptually. We are grateful to you and your collaborators for making us think through this matter, and we hope that we have given you sufficient acknowledgment.

There remains a question about what you should write in your paper. By making the correction, we don't want to take away what you wish to write about. It seems to me that the best strategy is the following: that you quote the offending passage about the constancy of the outcome function in the original GHZ paper, and at leisure show what is wrong with that statement. In other words, you give the argument that you present in your most recent letter. You may also legitimately point out that it was your correspondence with us that induced us to remove this offending passage in the GHSZ paper. The GHSZ paper is going to be read much more than the original GHZ paper, because of their respective loci, but that fact does not make your discussion obsolete. After all, the remarks we make about measure theory in GHSZ are very brief and perhaps even cryptic, whereas you discuss this matter at leisure. I hope that this suggestion will satisfy all desiderata: nothing will be criticized that ought not be criticized, nothing will escape criticism that ought to be criticized, and you will have plenty to write about in your paper.

I shall see Clifton at the PSA meeting. I hope you will be there too.

Reminder: your (16) is $\forall\phi\,\forall\lambda\,(A_\lambda(2\phi)=A_\lambda(0))$
your (16*) is $\forall\phi\; m\{\lambda: A_\lambda(2\phi)=A_\lambda(0)\}=1$.

With best wishes,

Abner

From Eqs. (12a,b) we obtain

$$A_\lambda(\phi)C_\lambda(\phi) = A_\lambda(0)C_\lambda(0), \tag{13a}$$

and from Eqs. (12a,c) we obtain

$$A_\lambda(\phi)D_\lambda(\phi) = A_\lambda(0)D_\lambda(0). \tag{13b}$$

A consequence of these is

$$C_\lambda(\phi)/D_\lambda(\phi) = C_\lambda(0)/D_\lambda(0), \tag{14a}$$

which can be re-written as

$$C_\lambda(\phi)D_\lambda(\phi) = C_\lambda(0)D_\lambda(0), \tag{14b}$$

because $D_\lambda(\phi)$ is ± 1 and hence equals its inverse, and the same for $D_\lambda(0)$. We then obtain from Eqs. (12d) and (14b)

$$A_\lambda(2\phi)B_\lambda(0)C_\lambda(0)D_\lambda(0) = -1, \tag{15}$$

which in combination with Eq. (12a) yields[15]

$$A_\lambda(2\phi) = A_\lambda(0) \quad \text{~~correct for all } \phi\text{~~}. \tag{16}$$

Eq. (16) is a quite surprising preliminary result. By itself, this equation is not mathematically contradictory, but physically it is very troublesome: for if $A_\lambda(\phi)$ is intended, as EPR's program suggests, to represent an intrinsic spin quantity, then $A_\lambda(\phi)$ and $A_\lambda(\phi+\pi)$ would be expected to have opposite signs. The trouble becomes manifest, and an actual contradiction emerges, when we use (11b) -- which until now has not been brought into play -- to obtain

$$A_\lambda(\phi+\pi)B_\lambda(0)C_\lambda(\phi)D_\lambda(0) = 1, \tag{17}$$

which in combination with Eq. (12b) yields

$$A_\lambda(\phi+\pi) = -A_\lambda(\phi). \qquad (18)$$

This results confirms the sign change which we anticipated on physical grounds in EPR's program, but it also contradicts the earlier result of Eq. (16) ~~that $A_\lambda(\phi)$ is constant in ϕ~~. We have thus brought to the surface an inconsistency hidden in premisses (i)-(iv). by letting $\phi' = 0$.

In the foregoing algebra, the argument of the function $B_\lambda(\phi_2)$ was fixed throughout to be 0, which shows that premisses (i)-(iv) are also inconsistent when applied to a system of three spin-½ particles. But we know from Bell's model (Appendix D) that the corresponding premisses are consistent for a pair of spin-½ particles. Correlations of three or more spin-½ particles involve at least one more degree of freedom than one finds in correlations of two spin-½ particles, and it is clear that the manipulation of an additional degree of freedom is essential to the exhibition of a contradiction.[16] The most significant feature of the new argument is the revelation that the EPR program cannot handle even the perfect correlations of quantum mechanics for systems of three or more particles. There is an irony in this result, in that perfect correlations are central to EPR's argument for the existence of states more complete than those of quantum mechanics.

IV. .... AND WITHOUT SPIN

In this Section we shall present a new gedankenexperiment, with three particles and without spin, for which the argument of the previous Section applies unchanged. The new system is an extension of a two-particle interferometer previously considered by Horne and Zeilinger[17] for exhibiting entangled states in momentum and position, and it resembles Mermin's[8] figure of a three-spin "gadget." Since the gedankenexperiment does not involve spin, it emphasizes, yet again, that Bell's theorem does not hinge on spin.[17] Since it employs only three particles, it emphasizes that the GHZ argument goes through for three particles. Finally, the new gedankenexperiment may be realizable in the laboratory.